cross over from no treatment to treatment was either aneurysm becoming symptomatic, a very subjective event, or the patients' request for treatment because of concerns related to their aneurysm. Very few patients, in all of the trials cited, crossed over from no treatment to treatment because of aneurysm size increases, which exceeded the limits defined in the protocols. Subjective, rather than objective, reasons for changing a randomized treatment assignment can undermine, to some degree, the validity of the trial.

While we appreciate the comments made to the editors reflecting a need to defend the particular trial cited, we note that the authors' conclusions are even more sweeping than ours. We do not claim that this problem is "universal" with, or that it negates the conclusions of, all randomized trials of such design. We simply reported that a crossover from no treatment to treatment was a problem in the author's experience with the PIVOTAL (Positive Impact of Endovascular Options for Treating Aneurysms Early) trial. In reviewing other trials of the same design, we found similar problems in the examples cited, (ie, in which AAA intervention was pitted against no treatment) the conclusions of such trials were jeopardized, if not made controversial, by the significant crossover rate. We are in no way generalizing or implying that randomized trials are not the "gold standard" of level I evidence, as the Letter to the Editor implies.

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Regarding "Endovenectomy of the common femoral vein and intraoperative iliac vein recanalization for chronic iliofemoral venous occlusion"

I recently had the privilege of reporting "Endovenectomy of the common femoral vein and intraoperative iliac vein recanalization for chronic iliofemoral venous occlusion"[1] in a patient with debilitating, chronic, postthrombotic occlusion of his iliofemoral venous system. The article stated that this was the first report of endovenectomy with endoluminal recanalization for patients such as these.

Although this is a true statement regarding the peer-reviewed literature, I saw this week that Dr Peter Gloviczki reported this procedure in the fifth edition of Rutherford's *Vascular Surgery* textbook.[2] I was not aware of his description in *Vascular Surgery* at the time the article was written. Dr Gloviczki discussed this technique with me approximately 2 years ago, which stimulated the team at the Jobst Vascular Center to pursue this treatment option in patients with chronic, postthrombotic, iliofemoral venous obstruction.

I want to recognize this important contribution of Dr Gloviczki and his team at Mayo Clinic as being the catalyst for our embarking on this technique and recognize that they were the first to publish this concept. Thank you, Peter!

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Regarding "Causes of late mortality after endovascular and open surgical repair of infrarenal abdominal aortic aneurysms"

We read with great interest a large (13,971 patients) retrospective cohort study by Goodney et al[1] of patients undergoing endovascular (EVAR) and open surgical repair (OSR) of abdominal aortic aneurysm. As expected, early mortality occurred in patients undergoing OSR, whereas deaths in patients undergoing EVAR occurred later. Although overall long-term mortality was similar in both cohorts (15.3% for EVAR and 15.9% for OSR at a median follow-up of 1.6 and 1.9 years, respectively), with an adjusted hazard ratio for mortality for patients undergoing EVAR of 0.98 (95% confidence interval, 0.90-1.07), the two unadjusted survival curves crossed at approximately 1.6 years of follow-up. A possible explanation could be that patients undergoing EVAR have more comorbidity and thus may be more likely to die after discharge than patients undergoing OSR.

To confirm whether crossing of survival curves occurs also in randomized cohorts (equal-risk patients), we performed a meta-analysis of three well-known trials: Endovascular Aneurysm Repair 1,[2] Dutch Randomized Endovascular Aneurysm Management,[3] and Open Versus Endovascular Repair.[4] The pooled cumulative survival rates were yielded by means of a strategy to combine survival data constructed by Pereira et al.[5]

In the first step, we redistributed in equal quantities at 1-month intervals patients censored at intervals >1 month. Next, we obtained the numbers of deaths for intervals of 1 month by using the patients at risk at the start of an interval, the redistributed censored patients, and the interval survival rates. We then calculated the Kaplan-Meier survival rates for each trial and each month of follow-up and used these rates as treatment effects.

In the second step, we calculated a within-trials variance for each monthly survival rate in each trial; next, we calculated a between-trials variance for each month. To obtain pooled measures of treatment effect for each month of follow-up, we used in the third step random-effects modeling.

Finally, the product of successive monthly pooled measures of treatment effect allowed us to obtain pooled measures of cumulative survival.

The pooled cumulative survival rates of EVAR and OSR were, respectively, 97.8% and 94.4% at 1 month; 95.3% and 92.2% at 1 year; 90.1% and 88.6% at 2 years; 82.8% and 83.4% at 3 years; 77.5% and 79.2% at 4 years; and 74.2% and 76.3% at 5 years (Fig). Two survival curves crossed at approximately 2.7 years of follow-up.

The results of our analysis suggest that crossing of survival curves of EVAR and OSR occurs even in randomized cohorts though the intersection is delayed by approximately one year as compared with the retrospective cohorts in the study by Goodney et al.[1] To confirm whether the differences in mortality after the crossing are increased, longer-term results of randomized trials would be needed.

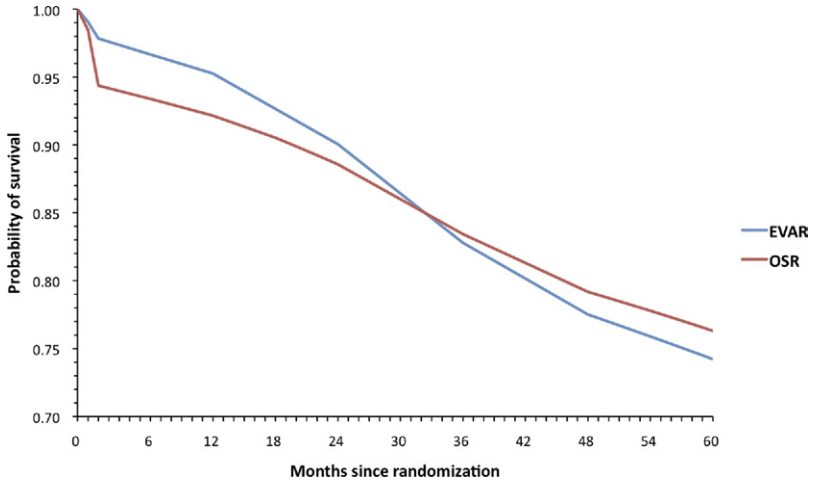

Fig. Pooled survival curves of endovascular (*EVAR*) and open surgical repair (*OSR*) of abdominal aortic aneurysm.

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Reply

In their Letter to the Editor, Takagi et al describe a pooled analysis of three large randomized trials of endovascular aneurysm repair (EVAR), wherein the survival curves of open surgical repair and EVAR crossed at approximately 2.7 years of follow-up. In our large cohort study of Medicare patients, we found that the survival curves crossed as well. However, this event occurred about a year earlier, at approximately 1.6 years.

Why might it be that patients who underwent endovascular repair experienced a more durable survival advantage in the Takagi pooled analysis of randomized trials as compared to our cohort study of realworld Medicare patients? We believe this represents an example of efficacy versus effectiveness.[1]

In the randomized trials, carefully selected patients underwent surgery in closely monitored centers of excellence, with specific audits of processes, outcomes, medical adjuncts, and strict characterization of pre- and postoperative risk. These trials clearly demonstrated the efficacy of EVAR in reducing perioperative morbidity and mortality.

Effectiveness, however, is established when a treatment works well in broad, generalizable settings. Our study—a national analysis of real-world outcomes in Medicare patients—demonstrates that EVAR is effective in reducing perioperative mortality, even when the closely monitored care present in randomized trials is not in place.

It would seem, therefore, that the difference in the care provided—between a randomized trial and the real world—plays some role in attenuating the survival advantage incurred by EVAR in the treatment of abdominal aortic anuerysms. This difference may be attributable to patient, surgeon, or hospital factors, across repair type as well as study type. Some of these factors may be evident in comparison of patient, surgeon, and hospital characteristics, and could theoretically be accounted for using risk adjustment. However, even if we could compare all measurable characteristics, adjusting for treatment bias in cohort studies is difficult, as several potential confounders are often unmeasurable, and even advanced statistical methods cannot always provide adequate risk adjustment.[2,3] In closing, we appreciate Dr Takagi's interest and believe their work reflects the important differences present between clinical trials and real-world implementation of new treatments for abdominal aortic anuerysms.

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